Measuring the Impact of the Education Guarantee Scheme on Schooling Outcomes of Women in India

Neha Raykar[*]

August 3, 2011

Abstract

In 1997, the state government of Madhya Pradesh (MP), India, launched the Education Guarantee Scheme to provide access to primary schools in the state's rural areas with the help of community participation. The scheme led to the rapid set up of over 25 thousand schools in a span of 5 years - an average of over 2 schools per 1000 rural children in MP between the ages of 6 to 15 in 1997. Exposure of an individual to the scheme was jointly determined by her state of residence as well as her age at the time of the intervention. Using the 2005-2006 round of the National Family Health Survey, this paper combines the two sources of exogeneity to estimate the impact of this unusual policy experiment on educational outcomes of women using a difference-in-differences estimation strategy. My findings reveal substantial and robust effects of the program on rural women's completed years of schooling and their probability of attending secondary school. Further, this increase in educational attainment is largely driven by the youngest cohorts of women in my sample, implying that the scheme was most effective in reducing the private costs of schooling for women who were just young enough to start primary school.

JEL Classification: I2, O15, O22

Key words: Human Capital Investment, Project Evaluation, Returns to Schooling

[*]Department of Economics, University of California, Riverside, Email: nrayk001@student.ucr.edu. I am deeply grateful to Anil Deolalikar, Jorge Agüero, and Mindy Marks for their valuable guidance. I also thank seminar participants at UC Riverside, Pacific Conference for Development Economics 2010, and Colgate University for their helpful comments.

1 Introduction

The potential contribution of education in stimulating growth and improving welfare in the developing world has been widely recognized by development policymakers and economists alike. Two of the eight Millennium Development Goals adopted by the United Nations Millennium Summit in September 2000 focus on universal primary education and gender equality at all levels of education by 2015. Macroeconomic literature emphasizes on the positive correlation between human capital development and economic growth amidst debate over the causal relationship between the two (Barro 1991, Mankiw, Romer, and Weil 1992, Pritchett 1996, Bils and Klenow 2000, Krueger and Lindahl 2001). A vast body of microeconomic literature empirically estimates the impact of investments in education on educational attainment as well as evaluates returns to education in developing countries using Ordinary Least Squares estimation, or more recently, using randomized trials and natural experiments [1]. Governments in developing countries have increasingly recognized the large returns to education, acknowledged the need for educational reform, and consequently incurred huge investments in education through various programs that reduce the private costs of schooling, provide incentives to encourage school attendance, or improve the quality of existing schools.

A common hindrance to educational attainment in the developing countries is the long distance that students often travel in order to attend school. A potential policy intervention is, therefore, to build additional schools to improve accessibility of the disadvantaged students and thereby reduce the private costs of schooling. Several empirical studies try to estimate the impact of distance to school on quantity of schooling using non-experimental variation in schools and households [2]. Such a methodology raises concerns about a potential omitted variable bias from unobserved factors, such as parental desire, that influence both distance to school as well as years of schooling

[1] See Glewwe (1996) for a detailed review of the literature

[2] Glewwe and Jacoby (1994) and Bommier and Lambert (2000) estimate the impact of distance to school on years of schooling in Ghana and Tanzania respectively and find a negative relationship between the two

attained. Recent studies that use "natural experiments" address this issue to some extent[3].

This paper uses variation arising from a unique policy intervention in a state of India to evaluate the impact of a rapid increase in the number of primary schools on women's educational outcomes. In 1997, the state government of Madhya Pradesh launched the Education Guarantee Scheme (EGS) whereby the state guaranteed to set up a school upon receiving a written demand from communities that did not have a school within 1 kilometer of habitation as long as there were at least 25 children between 6 to 14 years of age in tribal areas(at least 40 children in non-tribal areas). Although there have been several other centrally sponsored programs aimed at universalizing primary education at the national level, the EGS was unique to the state of MP in 1997. Between 1997 and 2003, 26,571 primary schools were started under the EGS. Population figures in Census of India 2001 suggest this was an average of over 2 schools per 1000 rural children in MP between the ages of 6 to 15 in 1997. Moreover, this rapid expansion of schools brought about by the scheme was an unusual experiment in improving schooling infrastructure with the hitherto untapped potential of community participation. Exposure of an individual to the EGS was jointly determined by her state of residence and by her age when the program was launched. Using the 2005-2006 cross-sectional wave of the National Family Health Survey (NFHS), I exploit variation arising from these two sources to measure the impact of the scheme on educational indicators for women. The main outcome variables considered in this paper are completed years of schooling and the probability of attending secondary school (grade 6 or above).

It must be noted that while the EGS was implemented for all children, boys and girls, of primary school-going age, there are two main reasons why the focus of my study is the girl child in particular. Firstly, while the impact of a school construction program on educational attainment and labor market returns for men has been extensively

[3] Duflo (2001) uses as a natural experiment, a rapid school construction intervention in Indonesia to evaluate the impact of increased schools on quantity of schooling and labor market outcomes by using variations in the exposure to the program across regions and cohorts of age. Also see Card and Krueger (2006) for a review of related studies

analyzed in the seminal paper by Duflo (2001), the impact of a similar intervention on women's educational gains, their returns to education, and other broader outcomes related to their welfare has not been previously studied. Secondly, in the context of the EGS, assessment of the scheme's performance has been restricted to field surveys and descriptive reports, with mixed evidence of its effects on educational status of women and other socially disadvantaged groups that were, prima facie, intended to be the largest beneficiaries of the scheme. To the best of my knowledge, there has been no study in the existing literature that offers a rigorous evaluation that would permit causal inferences of the scheme's impact on educational attainment of these historically marginalized groups. Moreover, as the related literature on developing countries suggests, I expect distance to school to be a larger impediment to education for the girl child vis-a-vis her male counterpart, hence the focus on women.

Evidence of robust program effects found in this paper is important in the context of other federal programs in the developing world aimed at improving educational outcomes of the poor. While the effects of conditional cash transfer programs on education such as those in Mexico (Progresa), Brazil (Bolsa Familia), and Nicaragua (Social Protection Network) have been widely studied, success of unconditional interventions in human capital development such as the INPRES program in Indonesia, Fundescola in Brazil, and the EGS in India could also be viewed as effective policy experiments for future educational policies. Moreover, this paper finds a large, positive effect of the scheme on poor rural women of MP - a finding that has testable implications for their non-labor market returns to schooling.

2 Institutional Background

Since India's independence, improving educational outcomes as the means to poverty alleviation, has been one of the foremost priorities of Indian policy makers. An important step in this direction came in the form of legislation in the 1950s and 1960s that decentralized the role of the central government through the establishment of Panchayats (local self-governments) in various states, whereby the village was empowered as the unit of administration. In 1992, the 73rd Amendment Act of the Indian Constitution provided constitutional status to the Panchayati Raj Institutions (PRI) and introduced a three-tier system of governance at village, block, and district levels to ensure people's participation in rural reconstruction. The state of Madhya Pradesh (MP) was one of the first states to enact the new system and form the PRI; the state government transferred powers and duties to these local institutions in areas such as primary education. Moreover, in 1994, the Government of India (GOI) launched the centrally-sponsored, donor-supported District Primary Education Program (DPEP)with a view to make primary schooling accessible to all children, and improve drop-out rates and enrollment gaps across gender and socioeconomic groups[4]. The DPEP was launched in two phases in the state of MP, and it fostered, among other things, the policy environment for further reforms in the area of primary education[5].

Further, in August 1994, the government of MP launched the Rajiv Gandhi Shiksha (Education) Mission headed by the chief minister of MP to supplement the state government's efforts to universalize primary education in the state. One of the first tasks of the Mission was to undertake a door-to-door survey and a mobilization campaign across the state in order to estimate the number of children enrolled in school and identify the reasons for large dropout rates in the state. The survey, conducted in 1996, called as the Lok Sampark Abhiyan (LSA), revealed large gaps in the outreach of schooling infrastructure, especially to the remote tribal areas of MP that were untouched by the school network. It also identified that the large number of dropouts could be attributed to students never being enrolled in school. It was the LSA's finding that an adequate primary school was absent in 32 percent of the state's villages that impelled the state government to launch the Education Guarantee Scheme (EGS) on January 1, 1997. The main objective of the Scheme was to make primary schooling ac-

[4]Under the program, districts with female literacy below the national average received DPEP funds for school construction, other amenities such as drinking water and toilets, teacher training, textbook revision, etc.

[5]For a detailed description of the DPEP, see see Clarke (2004) and Jalan and Glinskaya (2005). For a general overview of educational trends in India, see Kingdon (2007)

cessible, especially to children belonging to the socio-economically deprived sections of the population. Moreover, the Scheme was devised with the aim to forge collaboration between the government and the people to cooperatively undertake the responsibility of providing education.

The EGS rests on the idea of community demand and stimulates a 3-way partnership between the community, local government (Panchayat), and the state government. The Scheme allows communities in remote areas to submit a written request to the Panchayat demanding a school so long as there is no schooling facility available within one kilometer of the habitation. Under the Scheme, whenever there is a demand from such a tribal area with at least 25 children (or from a non-tribal area with at least 40 children) belonging to the age group of 6 to 14 years, the state government guarantees to provide a primary schooling facility within 90 days of the receipt of such a request. The community provides the physical start-up space for learning, identifies a local resident to be the teacher, and participates in the day-to-day functioning of schools through Village Education Committees (VEC) and Parents Teachers Associations (PTA). The local government overlooks the appointment of the teacher, while the state government provides for the teacher's remuneration, training and the teaching-learning material. EGS has been acknowledged as an innovative breakthrough in social sector planning. Within one year of launching the Scheme, 15,568 EGS schools came up in MP; as of June 30, 2003, the number of EGS schools has gone up to 26,571, catering to over 1.2 million children, 91 percent of which belong to socially disadvantaged groups residing in rural areas of the state.

Several descriptive and field studies in MP have corroborated the increased enrollment rates, lower drop-out rates, and increased literacy levels attributed to the success of the EGS. Gopalakrishnan and Sharma (1998) acknowledge the EGS as a cost-effective alternative to other centrally sponsored programmes aimed at universalizing primary education, and assert from a sample of 31 districts that, within a year since its launch, the Scheme helped increase enrollment rates of girls and children belonging to scheduled castes and scheduled tribes (SC-ST). According to the LSA household survey conducted in 2002-03 to assess the performance of EGS, the Gross Enrollment Rate (GER) in MP has increased from 76.5% in 1996 to 101.7% in 2002-2003, while the percentage of out of school children in MP has dropped from 29.3% to 6.2% between the same period [6]. Jalan and Glinskaya (2005), while evaluating Phase-I of the DPEP, acknowledge significant improvements in the states of MP and Rajasthan and suggest that MP's success with educational outcomes could be attributed to the EGS. Clarke (2004) conducts a field study in 2 districts of MP and observes that the EGS schools were qualitatively comparable to regular government schools and had the advantages of community participation, and parental and teacher motivation. Leclerque (2003) based on a field study acknowledges the progress in enrollment rates in MP owing to the EGS, however, raises doubts over the quality of education and teacher incentives. Despite the controversial success of the EGS, to the best of my knowledge, there has not been a rigorous evaluation of the EGS that would assess and quantify the causal impact of the Scheme on direct educational outcomes. My objective is to evaluate the effectiveness of the Scheme by exploiting the inter-state variation in program placement as well as the variation in exposure to treatment across cohorts induced by the timing of the intervention.

3 Empirical Framework

3.1 Identification Strategy and Data

Exposure to the scheme is jointly determined by the individual's state of residence and date of birth. After controlling for the state of current residence and the cohort of birth effects, the interaction between dummy variables for whether the individual was young enough to benefit from the scheme at the time of its launch in 1997, and whether the individual resides in the state of MP, should reflect the impact of the scheme on

[6] GER is the number of children currently enrolled in school, irrespective of their age, as a percentage of the total population of official schooling age. Ratios greater than 100 indicate a high number of over-aged students currently enrolled in school.

the outcome variable. Indian children typically attend 12 years of schooling; consisting of 5 years of primary school from 6-11 years of age, 3 years of middle school from 11-14 years of age, followed by 2 years of high school and higher secondary school each. Under the EGS, setting up a primary school in a community was conditional on a required number of children between 6-14 years of age. This implies that most children older than 14 years at the time of the program launch were too old to be affected by the program, although a few could benefit from late enrollment or grade repetition. Accounting for such irregularities, exposure to the program is a decreasing function of the student's age in 1997. As such, the scheme should have maximum impact on the youngest cohort of children who were 6 years old, and no impact on children who were older than 14 years in 1997. The control group therefore comprises of women who were 15 years or older at the time of the program launch.

The second source of variation in this strategy is the state of current residence of the women. It must be noted here that using the state of birth would be the ideal variable instead, as long as it is highly correlated to the state of educational attainment [7]. The NFHS dataset does not record individual's state of birth, making it impossible to identify differences, if any, between the women's current state of residence and the state where they were born. However, according to the 2001 Census of India, women from MP constitute about 4% of the total inter-state migrant population in India. Besides, 40% of women in my sample are never married and 43% report as having "always" lived in the current place of residence, indicating a fairly high correlation between the current state of residence and the state of birth. Therefore, I consider MP as the treated state and compare the educational achievements of women in MP vis-a-vis women in states that share geographical borders with MP[8]. Figure 1 shows the political map of India from the 2001 Census. As of 1997, the centrally located state

[7]The advantage of using the state of birth over the state of educational attainment is that if all women in the sample are born before the launch of the program, the state of birth is exogenous to placement into the program.

[8]MP was split into two states - MP and Chattisgarh - in the year 2000. Therefore, the NFHS round of 2005-2006 records data for MP and Chattisgarh separately. However, since the state was still integrated at the time of the launch of the EGS, I treat both Chattisgarh and MP as one state - MP. The EGS was still prevalent in Chattisgarh after the split.

of MP shared geographical borders with 7 other states: Uttar Pradesh (UP) on the north, Rajasthan and Gujarat on the west, Maharashtra and Andhra Pradesh (AP) on the south, and Orissa and Bihar (including the state of Jharkhand which was then a part of Bihar) on the east.

In this paper, I use cross-sectional data from Phase V of the Demographic Health Survey (DHS), also called as the National Family Health Survey (NFHS) in India. This is a nationally representative household survey dataset collected between 2005 and 2006. The survey collects information from women between the ages of 15 and 49 about educational attainment and marital status among other socio-economic characteristics, contraceptive use, fertility preferences, reproductive history, and anthropometric measures. Typically, the NFHS data is available at both, the individual as well the household level[9]. Individuals' record of the NFHS has two main variables for identifying educational status of the women interviewed. More specifically, women report their highest year of education completed and the highest educational level attended, based on which, the NFHS imputes two additional variables - completed years of education (based on the specific educational pattern of India) and educational achievement (categories that account for complete/incomplete educational levels). I use these two recoded variables as my main outcomes of interest.

As preliminary evidence of the impact of EGS on educational attainment, Figure 2 compares the general trends in average completed years of schooling for MP women between two independent cross-sectional rounds of the NFHS - Phase IV in 1998-1999 and Phase V in 2005-2006. While there is significant improvement in educational attainment of women for all age-groups up to 40 years across the two surveys, there is a sharp increase in completed years of schooling for the youngest cohort of women who were 15-19 at the time of the respective surveys. The average completed years of schooling for this cohort increases almost three times within a span of 7 years - a probable effect of the EGS intervention that affected the youngest cohort of women in

[9]Information on year of birth, state of current residence, and other socio-economic control variables is obtained from the individual records; these records are then matched with the household for household-level controls.

Phase V who were of primary school-going age at the time of the intervention in 1997.

Further, the identification strategy relies on the assumption that there were no systematic differences with respect to educational patterns across the comparison groups in the absence of the program. As preliminary evidence of pre-program parallel trends in educational attainment between the two comparison groups, I plot completed years of schooling against women's age in 1997 for MP and its neighboring states in Figure 3. As seen in the figure, in general, completed years of schooling for women in MP are lower than in the neighboring states. Amongst the treated group of women who were young enough (younger than 14 years in 1997) to be benefitted by the EGS, we see that for cohorts aged 6 to 8 years in MP, the completed years of schooling are above the average values for the corresponding cohorts in the neighboring states. Further, for the control group of women belonging to cohorts older than 14 years in 1997, the trends in completed years of schooling are largely parallel between MP and its neighboring states, except for cohorts in MP that were 19, 20, or 23 years old in 1997. This validates the identification strategy explained above. Moreover, it is also a preview of the impact of the EGS on the youngest cohorts of women in MP. To enthuse more confidence in the assumption that trends with regard to the main socio-economic indicators were parallel between MP and its neighboring states, I present descriptive statistics of some relevant socio-economic variables, both at the state level as well as the individual level obtained from the NFHS sample. The former are obtained from several official reports such as Central Statistical Organization (2001), Government of India (2001a), Government of India (1991), and Government of India (2001b). The descriptive statistics from the relevant NFHS sample refer to women of ages 15 to 30 at the time of the survey.

State-wise comparisons in the key indicators relevant to the identification assumption are presented in Table 1. Column (1) shows the performance of MP (treated group) compared to the average performance of the states that share a geographical border with it (control group) in column (2). The decennial population growth rate in MP is similar to the average population growth rate in its neighboring states. MP has a larger share of SC-ST population (37.8%) compared to the average share in the neighboring states. As of 1991, MP fares poorer than its neighbors with respect to Human Development Index, Female Infant Mortality Rate, Female Literacy Rate and Age-Specific Enrollment Ratio of girls belonging to the 6-11 age-group. It must be noted that of the 7 neighboring states, Maharashtra, Gujarat, and Andhra Pradesh perform better on most socio-economic indicators compared to the others. Historically, MP belongs to the group of "backward" states, that include Bihar, Rajasthan, UP, and Orissa.

Next, I present the mean, standard deviations, and t-values of relevant variables in the NFHS sample of women between 15 to 30 years used in the rest of the analysis. The average age at the time of the survey is 21 years in both groups. The share of the never married women in the entire sample is 36% in MP and 39% in the neighboring states. Proportion of Hindus is 90% in the treated group and 70% in the control group. 45% of the MP women in the sample live in urban areas whereas this share is 49% in the neighboring states. These statistics suggest that MP and its neighboring states are similar on an average with regards to the main demographic indicators. With regards to educational indicators of the women, 53% of the MP women between the ages of 15 and 30 have completed primary education. In the control group on an average, 58% of the women have completed primary education. The average completed years of schooling is 6 years in both the groups. The mean age of the household head (male of female) is about 44 years and 45 years in the treated and control group respectively, whereas the average completed years of schooling of the household head are 4.7 in MP and 4.9 years in the neighboring states. T-values in column (3) are large and statistically significant for all indicators except one; although this implies that the means between the comparison groups are significantly different from each other, in magnitude, the means are very similar as seen in columns (1) and (2).

Table 2 illustrates the basic idea underlying the identification strategy. This table compares, between MP and its neighboring states, the average completed years of schooling of the young cohort of girls aged 6 to 11 years in 1997 who could avail the primary schools set up under EGS, vis-a-vis girls older than 14 in 1997 who had little or no exposure[10]. The average completed years of schooling has increased over time; younger cohorts have more years of schooling than the older cohorts on an average. Moreover, educational attainment has increased more between cohorts in MP compared to its neighboring states. Besides, for both cohorts, the average number of years of schooling is lower in MP than in its neighboring states. This reflects the evidence that at the time of the launch of EGS, MP was one of the poorest states with regard to educational outcomes. The difference in differences can then be interpreted as the causal impact of the EGS, based on the assumption that in the absence of EGS, there would be no systematic differences in the educational attainment patterns between MP and the neighboring states.

From the table, it can be seen that the EGS, on an average, added 0.4 years of schooling for a young girl residing in MP. This difference-in-differences value is significant at the 10% level. The assumption underlying the identification strategy could be violated if educational patterns vary systematically across states or if other government schemes implemented during the same period of analysis systematically influence education of younger women in MP. These and other possibly confounding effects on the scheme's impact will be addressed in the following sections of the paper.

3.2 Model Specification

To allow for other sources of variation associated with educational attainment, we can generalize the above strategy using a regression framework.

$$Y_{ij} = \alpha + \beta S_j + \gamma C_i + \delta(S_j C_i) + \theta X_{ij} + \epsilon_{ij} \tag{1}$$

where Y_{ij} is the educational attainment of individual i in state j, S_j is dummy indicating whether the individual belongs to state j, C_i is a dummy indicating whether

[10]Girls between 1 and 6 years of age in 1997 would have been a better treatment group since they would be fully exposed to the scheme; however, the youngest cohort in the 2005-2006 NFHS is 6 years old in 1997. In that respect, my results could, at best, be a lower bound of the actual impact of the program.

the individual i belongs to the "young" cohort between 6 to 11 years in 1997. Coefficient of δ can be interpreted as the causal impact of EGS on educational outcomes; assuming the identification assumption is true, the δ coefficient is the average change in educational attainment that can be directly attributed to the scheme. ϵ_{ij} is the error term. X_{ij} is a vector of individual's time-invariant background characteristics such as education of the household head, type of residence, religion, caste, and the number of years respondents have lived in the current place of residence. Education of the household head is a proxy for the household's income; for instance, if household's socio-economic level as represented by the education of the household head, is positively (negatively) correlated to the likelihood of availing the EGS and positively correlated to the educational attainment of its women, omitting this variable could bias my estimates upward (downward). Similarly, if belonging to the SC-ST group is positively correlated to the likelihood of receiving EGS schools and negatively correlated to educational attainment, omitting the individual's caste from the regressions could bias the estimate downward, resulting in misleading conclusions about the true impact of the program. Such background characteristics are therefore included as covariates in the reduced-form estimation equation described above.

One of the limitations of the above specification is that it captures the combined effect of the program on all ages between 6 to 11 years that are "young" enough to be influenced by the scheme, vis-a-vis all ages that belong to the "old" cohort. However, it is of interest to analyze the age-group for which the scheme was most effective. To test this, the above equation can be generalized for a cohort-by-cohort analysis as following:

$$Y_{ij} = \alpha + \beta S_j + \gamma C_i + \sum_{k=6}^{20} \delta_k (S_j C_{ik}) + \theta X_{ij} + \epsilon_{ij} \tag{2}$$

where C_{ik} is now a dummy for each year of birth indicating whether the age of the individual i is k years in 1997. There are 15 such year-of-birth dummies for the individuals' ages in 1997. Individuals who are 21 years of age in 1997 belong to the omitted control group. This specification allows me to study the differential impact of the scheme on each cohort separately. The coefficient of interest is δ_k for each cohort k. I first present the main findings - the overall effect of the program for the average sample of women and then turn to heterogenous effects for each cohort of birth.

4 Results

4.1 Main Findings

The key result of this paper is shown in Table 3 which presents estimates of equation 1 for two outcome variables: completed years of schooling and the probability of attending secondary school. While the former is a continuous variable, I define the probability of attending secondary school as a variable that takes the value of one if the woman's reported educational achievement at the time of the survey is either incomplete secondary, complete secondary or higher education, and takes the value zero otherwise. In this table, I compare girls aged 6 to 11 in the year 1997 with those aged 15 to 21 in 1997 across MP and its neighboring states. It must be noted that although the eligibility for requesting an EGS school was to have a specified number of children between 6 and 14 within the community, the EGS schools were essentially primary schools catering to ages between 6 and 11; as such the impact of these schools on ages 12 to 14 is ambiguous. For instance, it could be argued that ages between 12 and 14 could benefit from the scheme if there are concerns about late enrollment and grade repetition for these ages and hence must be included in the treated group for true estimates of the scheme's impact on schooling. In my sample, only 2% of all the household members are reported as being "repeaters" at the time of the survey, although I have no information on late enrollment. It could also be argued that ages 12 to 14 are past the age for primary schooling and must therefore be excluded from the treated group of cohorts. I omit these ages in the initial set of results; however, as robustness checks, I include these cohorts in the treated group and then in the control group for 2 different experiments as shown in Section 5.1. Table 3 reports the estimated coefficients of the interactions between the cohort dummy and the dummy for whether the woman resides in MP at the time of the survey. Models 1, 2, and 3, as explained below, refer to the different sets of control variables used for each of the regressions. I present OLS estimates of completed years of schooling and the marginal effects of probit estimates of the probability of attending secondary school. Robust standard errors are clustered at the age level[11].

Column (1) of Table 3 shows that in the absence of any other controls for background characteristics X_{ij} in equation (1), exposure to the scheme increased average years of completed schooling by approximately half a year and increased the probability of attending secondary school by around 7 percentage points. Both the estimates are statistically significant. Model 2 adds controls for background characteristics such as caste, religion, education of the household head, and a dummy for whether the household is located in a rural area. Coefficients in column (2) suggest that after controlling for the background characteristics, the average completed years of schooling for women in MP who were young enough to be exposed to the program in 1997 increased by over 4 months while the probability of attending secondary school increased by 6 percentage points. While the estimated coefficients for completed years of schooling are not significantly different from zero, the estimates for the probability of attending secondary school are significant at the 5% level and can be interpreted as the causal impact of the EGS on the probability of attending a grade above grade 5.

One concern is that the women's state of educational attainment may be different from their current state of residence at the time of the survey; the gains in educational attainment being attributed to the EGS could be overestimated if the women belonging to the "treated" group are, in fact, migrants who completed primary education in a state different from MP. Although the NFHS collects no information about the women's state of residence at birth, women do report the number of years lived in their current place of residence. I use this variable as a rough measure of migration and include it

[11]In results not shown here, I have clustered the standard errors by age*state with no significant changes in the results.

as a covariate along with other background characteristics in Model 3. As seen from column (3) which adds this covariate to Model 2, exposure to EGS increases completed years of schooling by 0.3 years (not statistically different from zero) and the probability of attending secondary school by 5.5 percentage points (statistically significant at 5%).

Finding a statistically insignificant impact of the EGS on completed years of schooling for the average woman in MP is not surprising. The main goal of the EGS was to make primary schooling accessible to the remote rural areas of MP, largely occupied by the socio-economically disadvantaged castes and tribes that did not have access to a primary school and were thus disconnected from the state's existing school network. Hence, there is strong reason to believe that the impact of the EGS on women differed across the rural-urban divide as well as the caste divide within the state of MP. I therefore split the sample, first between rural and urban and then between SC-ST and non-SC-ST. Panel A of Table 4 reports the OLS estimates of the interaction term between the cohort dummy and state of residence dummy for completed years of schooling. As expected, the estimated coefficients of the interaction term are large and statistically significant for the rural sample across all 3 model specifications. After including the full set of controls as in column (3), exposure to the EGS increases completed years of schooling by 0.41 years for rural women in MP. This estimate is similar to the raw difference-in-difference value obtained in Table 2, clearly implying that the entire gain in the educational attainment of MP women could be attributed to the causal impact of the EGS in the *rural* areas of the state. Given that the average years of schooling is about 6 years in MP, this implies a 7 percent increase in completed years of schooling. On the other hand, the scheme has no significant impact on the urban sample of 11 thousand women. This is because the EGS was primarily designed to include rural areas into the state's school network.

As previously stated, documented achievements of the EGS also emphasize on the scheme's effect on the educational attainment of the socio-economically disadvantaged groups in the state, namely the SC-ST. Typically, EGS schools are set up at the community level and the socio-economic composition within the rural communities is largely homogenous; this implies that each school is attended and supported by a different socio-economic group that is dominant within the community (Clarke 2004). As such, beneficiaries of the EGS are heterogenous by their socio-economic status. To test this, I now split the original sample across the caste divide. About a third of the sample belongs to the SC-ST category, while the rest include "Other Backward Castes", other Hindus, and non-Hindus. Estimated coefficients of the interaction term show that, after including the full set of controls, the scheme increased completed years of schooling by 0.4 years for the marginalized group of SC-ST women in MP who were young enough to be exposed to the scheme in 1997; however, these estimates are not statistically different from zero. The scheme has a negligible and statistically insignificant impact on the non-SC-ST sample.

Panel B repeats the analysis to estimate the marginal effects of exposure to the EGS on the probability of attending secondary school. A similar pattern emerges. Exposure to the EGS increases the probability of attending grade 6 or above for rural MP women by approximately 6 percentage points, after controlling for the full set of background characteristics, whereas there is no significant effect on the urban sample. Further, dividing the sample between the SC-ST and non-SC-ST shows that exposure to the EGS had no significant impact on the probability of attending secondary school for SC-ST women in MP. Overall, Table 4 suggests that there is strong evidence of educational improvements for women in the rural areas of MP that can be attributed to the EGS. It must also be noted that due to data limitations, I cannot exactly identify the women who received treatment in the form of access to an EGS school. Clearly, not all women in the rural areas of MP were treated by the intervention. These results should therefore be interpreted as the impact of intention to treatment and at best, are a lower bound of the true impact of the program.

4.2 Heterogenous Effects

The results so far show the estimated impact of the EGS on average education of the collective group of young women aged 6 to 11 years in 1997 compared to women who were too old to be exposed to the program. In this subsection, I focus on the impact of the scheme on each cohort so as to identify the cohort(s) for which the scheme was most effective. In particular, I estimate equation (2) above, which, in effect, is a generalized extension of equation (1) to enable a cohort-by-cohort analysis. If the identification strategy is credible, the impact of the program should be a decreasing function of the women's age; as such, estimated coefficients of the interaction term should fluctuate around zero for cohorts that are older than the primary school-going age. To obtain more precise estimates of the program's impact, I employ a restricted estimation of the heterogenous effects of the program by redefining the control group as all individuals aged 15 to 21 in 1997. This strategy can be expressed in the form of the following modification of equation (2) described earlier:

$$Y_{ij} = \alpha + \beta S_j + \gamma C_i + \sum_{k=6}^{14} \delta_k (S_j C_{ik}) + \theta X_{ij} + \epsilon_{ij} \tag{3}$$

Instead of testing for whether the δ_k coefficients are zero for each cohort older than 14 years in 1997, this equation is a more efficient way to analyze heterogenous effects of the program across cohorts (Duflo 2001). Results for the outcomes of completed years of schooling and the probability of attending secondary school for the rural sample are presented in Table 5. This table shows the coefficients of the interaction term between the dummy for being a given age in 1997 and the state of residence dummy for the full set of controls used in section 5.1. The coefficients of interactions between being a given age in 1997 and the state dummy are the largest for the youngest rural cohort of 6 year old girls and the effects are statistically significant at the 1% level for both outcomes. Exposure to the EGS in the rural areas of MP increased the average completed years of schooling by over 1.5 years and the probability of attending secondary school by 25 percentage points for the youngest cohort of rural girls aged 6 years at the time of the program launch. The effects of the program on completed years of schooling as well as the probability of attending secondary school become smaller for older cohorts and become statistically insignificant for cohorts older than 8 years in 1997. The coefficients generally decrease with age except for an increase at the age of 13. The effect is negative for the cohort that is 12 years old in 1997. These results indicate that the identification strategy is valid. Moreover, these effects imply that the costs of starting primary school are highly non-linear; first-time enrollment into primary school becomes very costly at older ages for this sample of women in rural India. This is an important observation for policy action since this suggests that providing increased access to primary schools may not have the desired impact on enrollment of women who are slightly older than the first couple of grades of primary school.

5 Robustness Checks

In this subsection, I present a few falsification and robustness tests that confirm the validity of the identification strategy such that increases in educational outcomes as observed in the previous section can be attributed to the EGS alone rather than to pre-existing trends across states. The identification strategy relies on the assumption that educational patterns do not vary systematically across states. For instance, evidence of rapid increases in education in MP vis-a-vis its neighboring states before the EGS was launched, would invalidate the impact of the EGS as causal. Although there has been evidence that MP was one of the most backward states with regard to educational indicators prior to this scheme, I can conduct a control experiment to test the above concern. Rural cohorts that are older than 14 years of age in 1997 were not exposed to the program. Thus, if I compare educational attainment of cohorts that are 15 to 20 years of age versus cohorts 21 to 25 years in 1997, in the absence of such trends, the educational increments between these 2 groups should not systematically differ across the states. Table 7 shows the results from this control experiment for both outcomes across three sets of controls.

In Table 6, rural cohorts between the ages of 15 to 20 in 1997 form the pseudo "treated" group whereas cohorts between 21 to 25 form the control group. Models 1, 2, and 3 refer to the three sets of controls as discussed earlier. If, prior to the launch of the EGS, temporal growth in educational attainment was systematically higher in MP vis-a-vis its neighboring states, the coefficients of the interaction between the cohort dummy and the state dummy must be positive and statistically different from zero for both outcomes across the three specifications. In Table 7, coefficients in Model 1 (without controls) reveal a very small but statistically insignificant "effect" for both outcomes. Adding more controls, coefficients in Models 2 and 3 reveal negligible "effects" (even negative in case of completed years of schooling) that are never statistically significant, suggesting that there are no systematic inter-state differences in educational patterns prior to the scheme.

The difference-in-difference estimates above can be interpreted as the impact on educational attainment of exposure to the EGS for women residing in MP vis-a-vis women residing in MP's neighboring states that collectively form the control group. However this control group consists of a heterogenous mix of 7 states that differ significantly from each other on socio-economic grounds. In particular, MP is similar in characteristics to the states of Bihar, Rajasthan, and UP. Historically, these four states were together termed as BIMARU ("sick") states due to their characteristically low levels of GDP and other socio-economic indicators. Later, the state of Orissa was also added to this list. On the other hand, the states of Gujarat, Maharashtra, and AP fare better then these states on most outcomes. Therefore, as a robustness check, I now use those neighboring states that are similar to MP as a collective control group to measure the performance of EGS.

Results for the two outcome variables are presented in columns (1) and (2) of Table 7 respectively. Each row refers to the group of states that has been used as the control group and each cell in columns (1) and (2) represents a separate regression estimating the coefficient of the interaction term between the cohort dummy and the state of residence dummy for the full set of controls described earlier. The analysis is restricted to the rural sample and the number of observations are presented in column (3). Results are generally consistent with previous findings of educational improvements attributable to the EGS. For instance, as seen from columns (1) and (2), exposure to the EGS for young rural women in MP has a positive and statistically significant impact on their completed years of schooling and their probability of attending secondary school vis-a-vis their counterparts in Bihar, Rajasthan, and UP together. The point estimates are very similar to the original findings for the rural sample in Table 4. Adding Orissa to the control group increases the point estimates for both outcomes. Next, I include all the other states together as a control group. The point estimates are still large and statistically significant, implying that the results are large and robust to alternative specifications of the control group.

Finally, I examine whether the identification strategy is sensitive to alternative definitions of the treatment group. Table 8 presents the main findings in Section 5.1 by redefining the age-group that is exposed to the EGS to individuals who were 12 to 14 years old in 1997. Once again, the analysis is restricted to the rural sample. In panel A, I redefine the cohort dummy to be equal to one if the individual is between 6 to 14 years of age in 1997, and is equal to zero otherwise. As before, the control group comprises of individuals between 15 to 21 years in 1997. In Panel B, cohort dummy is equal to one if the individual is between 6 to 11 years of age in 1997, and is equal to zero otherwise. The control group is redefined to comprise of individuals between 12 to 21 years in 1997. The magnitude of the estimated coefficients of interaction in Panel B are similar to the main findings in Table 4 for both outcomes of interest for the rural sample, indicating that rural women who were 12 to 14 years at the time of the launch of EGS were too old to be exposed to the treatment and therefore including them in the treatment group as in Panel A would underestimate the true impact of the scheme. This justifies excluding these cohorts from the treated group; it also reinforces the earlier findings that first-time entry into primary school made available by such a scheme, becomes costlier for older cohorts who were previously never enrolled.

6 Discussion and Conclusion

6.1 Discussion

In summary, the results suggest that the EGS increased educational attainment of rural women exposed to the scheme in MP. However, to avoid misinterpreting the results in a more general context, certain caveats must be considered. As previously mentioned, in the decade of the 1990s, India witnessed several initiatives by the central government in partnership with the state governments aimed at universalizing primary education. If such an educational intervention ran simultaneously with the EGS during my period of analysis, differentially benefitting younger women in MP, my results will be confounded by the correlation between such a program and the EGS. One such scheme was the DPEP launched in 1994 by the central government of India. Although the DPEP was nationally introduced at the district level, MP received a larger share of the DPEP funds owing to its poor educational records. Since the dataset I use in this paper offers no retrospective information on what type of school the women attended, I cannot disentangle the effects of the EGS from the DPEP. However, evidence from existing government reports suggests that in MP, funds received from the DPEP were diverted to build EGS schools, thus leading the 2 schemes to merge into the EGS (Clarke 2004). Further, compared to the 26,571 EGS schools that were set up between 1997 and 2003, the number of regular primary (non-EGS) schools opened in MP was a mere 4209 between 1994 and 1998; this clearly suggests that the EGS was, more aggressive and intensive in approach, and can account for the large effects on educational attainment seen in this paper. More importantly, since the DPEP was launched in 1994 and the EGS in 1997, if the results in this paper were driven by the DPEP instead of the EGS, the impact should have been evident on older girls between 9 and 12 years of age in 1997 who were benefitted by the launch of DPEP in 1994. My results show that the increase in educational attainment only persisted up to the cohort aged 8 in 1997, which confirms that these improvements were a result of the EGS and not the DPEP.

The EGS relies heavily on community participation besides the state government's assistance in providing educational infrastructure. Therefore, the apparent success of the EGS hinges largely on the motivation and responsibility of the communities that initially request for a school and later manage and supervise its functioning. If communities in the state of MP have been systematically more motivated and responsible than their counterparts in the other states, the effects on educational attainment in MP could be erroneously attributed to the EGS rather than to the characteristics of its communities. The aim of this paper is to assess the impact of the EGS experiment in MP, while acknowledging that such an experiment may be unique to this state and its effects may not be generalized to other less favorable settings.

As suggested earlier, the NFHS round of 2005-2006 does not provide any information on migration of the individuals interviewed. Therefore, it does not allow the identification of the state where the individuals were born and/or educated, which could be distinct from the reported state of current residence. This implies that my identification strategy is based on the assumption that there is no significant inter-state migration of women. This is not entirely implausible; according to the Census of India 2001, women from MP constitute about 4% of the total inter-state migrant population in India. Further, as seen from my findings, using a rough measure of migration does not significantly alter the impact of the EGS on the educational attainment of the "treated" group, indicating that migration may not be of a critical concern. Another important caveat is that the NFHS data is not available at the district level, thus making it impossible to identify individuals living in districts located along the borders of MP. If children residing in a neighboring state gain access to an EGS school situated in close proximity across the border in MP, it would imply that there are individuals in the comparison group who are possible beneficiaries of this scheme. Excluding them from the treated group in the current analysis implies that the apparent gains in educational attainment seen in the previous sections are, in fact, a lower bound.

This paper offers no measure of the qualitative gains in education from the scheme. The NFHS dataset does not record information on any indicators of qualitative educational achievement; I cannot ascertain whether the increase in quantity of education attributed to the EGS is matched with qualitative improvements, if any. Existing field studies on the EGS provide mixed evidence on the standards of education in the EGS schools based on indicators such as student-teacher ratios, teacher motivation, and textbook curriculum (Leclerque 2003, Clarke 2004). This paper's contribution to the existing literature is to rigorously evaluate the effect of a policy aimed at reducing the cost of primary education on the *quantitative measures* of women's education. Finally, this paper does not consider the long-run impact of the EGS. Large changes in educational attainment in the state of MP could potentially increase the supply of educated women in the labor market, decrease the returns to education and thereby alter the demand for education. This paper ignores the general equilibrium effects of the scheme.

6.2 Conclusion

In this paper I exploit a unique policy experiment in India to evaluate the impact of investments in educational infrastructure on educational attainment of women. The EGS was an unusual initiative by the state government that led to rapid growth of primary schools in the rural areas of MP - an average of over 2 schools per 1000 rural children of ages 6 to 15 in 1997 - in collaboration with communities and local self-governments. Using cross-sectional data from the 2005-2006 round of the NFHS, I find a positive effect of the EGS on women's completed years of schooling as well as the probability of attending secondary school. On average, the estimates suggest that exposure to the scheme in the rural areas of the state increased completed years of schooling by a quarter of a year and increased the probability of attending secondary school by around 6 percentage points. The program effects are driven by the youngest cohorts of women who were 6-8 years old at the time of the program launch, suggesting that the EGS played a crucial role in reducing the private costs of primary school for women who were just young enough to start schooling. This suggests that the costs of schooling in these rural areas may be highly non-linear in nature; starting school at older ages is very costly in these rural areas of MP despite the provision of new schools. A battery of robustness checks confirm the interpretation of the estimates as the causal impact of the EGS on educational attainment; although these results must be interpreted with caution in a more general setting.

These results are especially important in the Indian context. Since 1960, Indian policy makers have endeavored at providing free and compulsory education to all children up to the age of 14 and recently recognized it, in 2009, as a constitutional right of every child. However, this goal is still elusive despite the enactment of a wide array of schemes and allocation of large financial resources by governments at the central as well as state level. India's budgetary allocation for education forms 3.8% of its Gross Domestic Product (GDP) as of 2005-2006. The National Policy on Education established in 1986 envisages an increase in this share to 6% of the GDP in order to meet the country's educational targets. Amidst debate over the adequacy of financial resources toward universalizing primary education, assessment of existing policies becomes crucial to policy makers. This paper evaluates the impact of one such scheme introduced in the state of MP, the EGS, which was uniquely premised on decentralized management based on community participation. While the existing literature on such programs is largely of a descriptive nature, rigorous evaluation that would permit causal inferences about the effectiveness of such alternative interventions could contribute toward paving the way for designing potent educational policies in the future.

Positive and robust effects of the EGS on educational attainment of poor women in rural areas of MP suggest that building primary schools with the assistance of community participation can be a profitable exercise in improving schooling infrastructure, especially for marginalized women. Existing empirical literature suggests evidence of higher returns to schooling for women vis-a-vis men - an inference that has testable implications for this paper. A relatively less explored area of research is the use of a natural experiment to evaluate returns to schooling for non-labor market outcomes for poor women who do not participate in the formal labor market. Moreover, it is also of interest to assess the positive spillover effects of increased educational attainment on a broader range of outcomes related to women's welfare. For instance, increased education may affect marital and/or fertility decisions of women or may have intergenerational effects on outcomes related to their children. Analyzing these indirect consequences of the EGS will be the scope of future research.

References

Barro, R. (1991): "Economic Growth in a Cross-Section of Countries," *Quarterly Journal of Economics*, 106(2), 407–443.

Bils, M., and P. J. Klenow (2000): "Does Schooling Cause Growth," *American Economic Review*, 90(5), 1160–1183.

Bommier, A., and S. Lambert (2000): "Education Demand and Age at School Enrollment in Tanzania," *Journal of Human Resources*, 35(1), 177–203.

Card, D. E., and A. B. Krueger (2006): "Schools, teachers, and educational outcomes in developing countries," *Handbook of the Economics of Education*, edited by Eric A. Hanushek and Finis Welch. Amsterdam: North Holland, 943–1017.

Central Statistical Organization (2001): *National Accounts Statistics*. CSO, New Delhi.

Clarke, P. (2004): "Education reform in the Education Guarantee Scheme in Madhya Pradesh, India, and the Fundescola Program in Brazil," *World Development Report*, 26948.

Duflo, E. (2001): "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment," *American Economic Review*, 91(4), 795–814.

Glewwe, P. (1996): "Schools and Skills in Developing Countries: Education Policies and Socioeconomic Outcomes," *Journal of Economic Literature*, 40(2), 436–482.

Glewwe, P., and H. Jacoby (1994): "Student Achievement and Schooling Choice in Low-Income Countries: Evidence from Ghana," *Journal of Human Resources*, 29(3), 843–864.

Gopalakrishnan, R., and A. Sharma (1998): "Education Guarantee Scheme in Madhya Pradesh: Innovative Step to Universalise Education," *Economic and Political Weekly*, 33(39), 2546–2551.

Government of India (1991): *Census of India 1991.* Office of the Registrar General, New Delhi.

——— (2001a): *Economic Survey 2000-2001.* Ministry of Finance, New Delhi.

——— (2001b): *National Human Development Report 2001.* Planning Commission of India, New Delhi.

Jalan, J., and E. Glinskaya (2005): "Improving Primary School Education in India: An Impact Assessment of DPEP-Phase I," *World Bank, Washington D.C.*, Poverty Analysis - Key Readings.

Kingdon, G. G. (2007): "The Progress of School Education in India," *Oxford Review of Economic Policy*, 23(2), 168–195.

Krueger, A. B., and M. Lindahl (2001): "Education for Growth: Why and for Whom?," *Journal of Economic Literature*, 39(4), 1101–1136.

Leclerque, F. (2003): "Education Guarantee Scheme and Primary Schooling in Madhya Pradesh," *Economic and Political Weekly*, 38(19), 1855–1869.

Mankiw, N. G., D. Romer, and D. Weil (1992): "A Contribution to the Empirics of Economic Growth," *Quarterly Journal of Economics*, 107(2), 407–437.

Pritchett, L. (1996): "Where Has All the Education Gone?," *World Bank Policy Research Working Paper*, 1581.

Table 1: Descriptive Statistics

	Madhya Pradesh (1)	Neighboring States (2)	t-values (3)
Background Indicators at the State Level:			
Decennial Growth Rate of Population (1990-2001)	24.34	22.48	
Proportion of SC-ST in Total Population (1991)	37.81	25.22	
Human Development Index (1991)	0.33	0.37	
Female Infant Mortality Rate (1991)	136	85	
Female Literacy Rate (1991)	28.85	53.86	
Age-Specific Enrollment of Girls in 1991 (6-11 years)	40.90	42.59	
Household Survey Indicators: Women aged 15-30 at the time of survey			
Current age	21.81	21.62	2.797
	(4.46)	(4.37)	
Proportion of women never married	0.36	0.39	-4.074
	(0.48)	(0.49)	
Proportion of hindus	0.90	0.79	22.357
	(0.30)	(0.41)	
Proportion living in urban areas	0.45	0.49	-5.234
	(0.50)	(0.50)	
Proportion of women without schooling	0.30	0.30	0.000
	(0.46)	(0.46)	
Proportion above primary education	0.53	0.58	-6.565
	(0.50)	(0.49)	
Completed years of schooling	6.02	6.41	-5.433
	(4.60)	(5.13)	
Age of the household head	43.69	45.06	-6.456
	(13.87)	(13.95)	
Household head's completed years of schooling	4.76	4.90	-2.424
	(3.75)	(3.91)	
Observations	5,185	24,536	

Notes: Age-Specific Enrollment Ratio = (Estimated enrollment in an age group ÷ Estimated child population in that age group) × 100. State-level background indicators are obtained from official reports by Central Statistical Organization (2001), Economic Survey of India (2001), Census of India (1991), and National Human Development Report (2001). Household-level indicators are obtained from the NFHS sample of women aged 15 to 30 at the time of the survey. Standard deviations are in parentheses.

Table 2: Average completed years of schooling by state and cohort

	Treated State (1)	Neighboring (Control) States (2)	Difference (3)
Cohort aged 6 to 11 in 1997	6.53	6.61	-0.088
	(0.084)	(0.043)	(0.100)
Cohort aged 15 to 21 in 1997	5.44	5.96	-0.524
	(0.116)	(0.054)	(0.129)
Difference	1.09	0.65	0.440
	(0.145)	(0.069)	(0.165)

Notes: Standard errors are in parentheses.

Table 3: Impact of EGS on Educational Attainment: Coefficients of interactions between cohort dummy and the state of residence dummy: Women aged 6-11 or 15-21 years in 1997

	Model 1 (1)	Model 2 (2)	Model 3 (3)
Panel A: Completed years of schooling			
OLS estimates	0.546*	0.363	0.305
	[0.272]	[0.222]	[0.209]
(R-squared)	0.03	0.37	0.39
Panel B: Probability of attending secondary school			
Marginal effects	0.068**	0.062**	0.055**
	[0.027]	[0.029]	[0.028]
Control variables:			
Individual age dummies	Yes	Yes	Yes
Household characteristics†	No	Yes	Yes
No. of years in current residence	No	No	Yes
Observations	23,749	23,749	23,749

Notes: Robust standard errors are in brackets and are clustered at the age level. * denotes significance at 10%; ** at 5% and *** significance at 1%. Treatment group: cohorts aged 6 to 11 in 1997; Control group: cohorts aged 15 to 21 in 1997. Each cell represents a separate regression. † include type of residence(urban/rural), religion, caste, and education of the household head.

Table 4: Heterogenous Effects of the Education Guarantee Scheme: Coefficients of Interaction between the cohort dummy and the state of residence dummy

	Model 1 (1)	Model 2 (2)	Model 3 (3)	Observations (4)
Outcome: Completed years of schooling				
Rural Sample	0.497*	0.485**	0.414*	12,289
	[0.233]	[0.215]	[0.202]	
Urban Sample	0.326	-0.116	-0.143	11,460
	[0.526]	[0.432]	[0.420]	
SC-ST Sample	0.595*	0.562*	0.366	6,542
	[0.302]	[0.270]	[0.265]	
Non-SC-ST Sample	0.407	0.100	0.098	17,207
	[0.345]	[0.290]	[0.277]	
Outcome: Probability of attending secondary school (marginal effects)				
Rural Sample	0.072***	0.067**	0.058**	12,289
	[0.026]	[0.028]	[0.026]	
Urban Sample	0.043	0.013	0.011	11,460
	[0.038]	[0.037]	[0.034]	
SC-ST Sample	0.066*	0.061	0.039	6,542
	[0.035]	[0.038]	[0.037]	
Non-SC-ST Sample	0.068**	0.047	0.047	17,207
	[0.033]	[0.036]	[0.033]	
Control variables:				
Individual age dummies	Yes	Yes	Yes	
Household characteristics†	No	Yes	Yes	
No. of years in current residence	No	No	Yes	

Notes: Robust standard errors are in brackets and are clustered at the age level. * denotes significance at 10%; ** at 5% and *** significance at 1%. Treatment group: cohorts aged 6 to 11 in 1997; Control group: cohorts aged 15 to 21 in 1997. Each cell represents a separate regression. Non-SC-ST include "Other Backward Castes", other Hindus, and non-Hindus. † include type of residence(urban/rural), religion, caste, and education of the household head.

Table 5: Heterogenous effects of the EGS on Completed Years of Schooling for the Rural Sample: Coefficients of the Interaction between Age in 1997 and the State of Residence Dummy

Age in 1997	Completed years of schooling (1)	Probability of attending secondary school (2)
6	1.547***	0.252***
	[0.193]	[0.022]
7	0.613**	0.080***
	[0.252]	[0.023]
8	0.517**	0.038
	[0.215]	[0.028]
9	0.06	0.038
	[0.384]	[0.055]
10	0.33	0.037
	[0.329]	[0.034]
11	0.221	0.047
	[0.204]	[0.036]
12	-0.241	-0.014
	[0.279]	[0.022]
13	0.463*	0.074**
	[0.254]	[0.029]
14	0.179	-0.018
	[0.344]	[0.024]
Control variables:		
Household characteristics†	Yes	Yes
No. of years in current residence	Yes	Yes
Observations	15,302	15,302

Notes: Robust standard errors are in brackets and are clustered at the age level. * denotes significance at 10%; ** at 5% and *** significance at 1%. Omitted control group: rural cohorts aged 15 to 21 in 1997. Regressions for both outcomes include the full set of controls. † include religion, caste, and education of the household head.

Table 6: Control Experiment for Rural Women Aged 15-25 in 1997: Coefficient of Interaction between Cohort Dummy and the State of Residence Dummy

	Model 1 (1)	Model 2 (2)	Model 3 (3)
Youngest Cohort: Women 15 to 20 years old in 1997			
Outcome:			
Completed years of schooling	-0.040	-0.126	-0.130
	[0.303]	[0.243]	[0.242]
Probability of attending secondary school (marginal effects)	0.012 [0.032]	0.000 [0.025]	0.000 [0.025]
Control variables:			
Individual age dummies	Yes	Yes	Yes
Household characteristics†	No	Yes	Yes
No. of years in current residence	No	No	Yes
Observations	18,599	18,599	18,599

Notes: Robust standard errors are in brackets and are clustered at the age level. * denotes significance at 10%; ** at 5% and *** significance at 1%. † include religion, caste, and education of the household head.

Table 7: Robustness Check with Each Neighboring State as the Control Group

States	Completed years of schooling (1)	Probability of attending secondary school (2)	N (3)
Bihar + Raj. + UP	0.423*	0.058**	8,112
	[0.203]	[0.027]	
Bihar + Raj. + UP + Orissa	0.565**	0.076***	9,388
	[0.196]	[0.027]	
All India	0.498*	0.075**	28,109
	[0.236]	[0.030]	

Notes: Robust standard errors are in brackets and are clustered at the age level.* denotes significance at 10%; ** at 5% and *** significance at 1%. Each row indicates the difference-in-difference estimates using the corresponding state as the control group and MP as the treatment group; Cohorts aged 6-11 or 15-21 in 1997. Estimation is restricted to the rural sample. Estimation includes full set of controls including household characteristics (religion, caste, and education of the household head) and number of years lived in current place of residence. "Raj." refers to Rajasthan. "All India" includes all states except MP and Chattisgarh as the control group.

Table 8: Robustness Checks for Alternative Definitions of Treatment and Control Groups

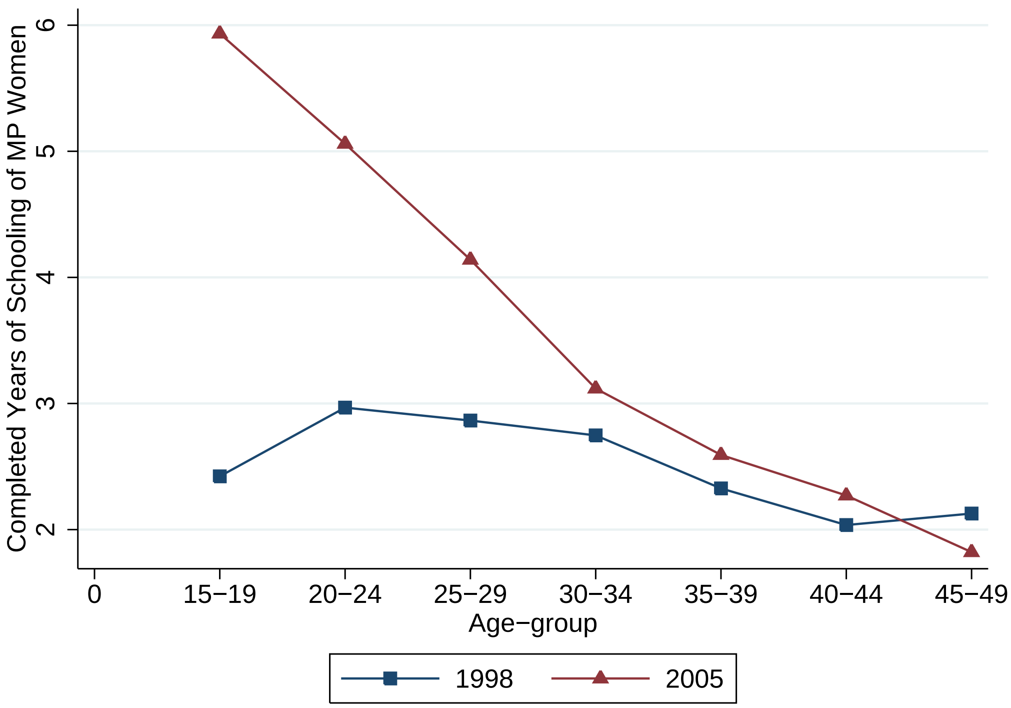
	Model 1 (1)	Model 2 (2)	Model 3 (3)
Panel A: including cohorts 12 to 14 in the treatment group			
Outcome:			
Completed years of schooling	0.352	0.332	0.300
	[0.257]	[0.222]	[0.204]
Probability of attending	0.052*	0.046*	0.042*
secondary school (marginal effects)	[0.027]	[0.026]	[0.024]
Panel B: including cohorts 12 to 14 in the control group			
Outcome:			
Completed years of schooling	0.480**	0.468**	0.369**
	[0.205]	[0.177]	[0.161]
Probability of attending	0.069***	0.064***	0.053**
secondary school (marginal effects)	[0.021]	[0.023]	[0.021]
Control variables:			
Individual age dummies	Yes	Yes	Yes
Household characteristics†	No	Yes	Yes
No. of years in current residence	No	No	Yes
Observations	15,302	15,302	15,302

Notes: Robust standard errors are in Brackets and are clustered at the age level. * denotes significance at 10%; ** at 5% and *** significance at 1%. Estimation is restricted to the rural sample. Each cell represents a separate regression. † include religion, caste, and education of the household head.

Figure 1: Political Map of India

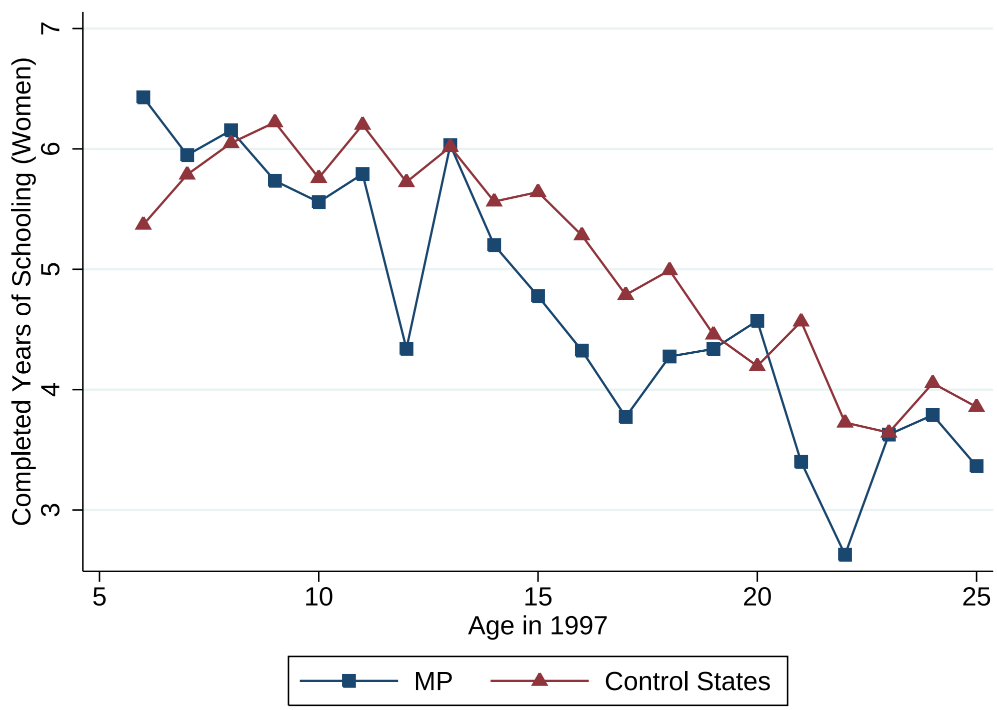
Source: Census of India, 2001

Figure 2: Trends in completed years of schooling (MP)



Source: NFHS 1998, 2005. Note: Figure 2 shows women's completed years of schooling for age-groups of five for the state of MP across two rounds of the NFHS.

Figure 3: Trends in completed years of schooling - MP versus control states



Source: NFHS 2005-2006. Note: Figure 3 shows the completed years of schooling for different cohorts of women in 1997 for the treated state of MP as well as control states that comprise of seven states that share a geographical border with MP.